Reviewer 1

General comments (overall quality of the discussion paper)

The authors have presented a nicely prepared study that applies hydrological simulations driven by different meteorological forcings for past and future climate conditions to estimate changes in high- and low-flow conditions for different rain- and melt-dominated catchments in Switzerland. Thereby, different approaches for estimating high- and lowflow regimes are applied and compared, discussing the individual strengths and weaknesses of each approach. A total number of 39 realizations of three different IPCC representative concentration pathways (8 x RCP2.6, 13 x RCP4.5 and 18 x RCP8.5) is compared to reflect the bandwidth of potential change in future discharge conditions in the catchments considered. As the knowledge on potential changes in highland low-flow conditions is of high societal, economic and ecological importance, the present study is of high scientific relevance with the thematic focus fitting nicely in the thematic scope of HESS. The formal requirements for publishing are almost fully met as reflected in the low number of technical corrections suggested in the following.

My major concerns relate to the comparisons of current and future discharge conditions. While the hydrological simulations representing the data basis for the estimation of current flow conditions are driven by meteorological observations, the simulations for future discharge conditions are based on climate conditions simulated by different GCM-RCM combinations. Although the downscaling approach of quantile mapping has been applied to statistically correct biases in the climate simulations for past and future conditions, the meteorological conditions in the observational data set applied for the hydrological simulations for the past are still not identical to the RCM simulated climate simulations for the past. This induces biases when it comes to the comparison of past and future discharge conditions calculated on the basis of meteorological data from the two different origins. Differences in the meteorological data sets can be expected to occur at shorter time scales (as highly relevant for extreme weather and hydrological conditions) as well as with respect to the interdependency of various meteorological variables, e.g. temperature, humidity and precipitation (such inter-variable consistency is not conserved applying quantile mapping). The existing differences in the meteorological observations and climate simulations with their respective hydrological effects for the past become clearly evident looking at the differences between the hydrological simulations achieved using meteorological observations and simulations in the case of both mean and extreme conditions in Figure 6. Moreover, calibration of the hydrological model seems to be carried out using meteorological observations for the past. While less important compared to the issue described before, calibration can indirectly compensate deficiencies in the meteorological input applied during calibration. Applying the same calibrated parameter set using different meteorological input (with very likely different deficiencies, e.g. those related to the quantification of precipitation) might lead to inconsistencies in the model results. A way to avoid the inconsistencies arising from different meteorological input for the past and future would be to correct the RCM simulations for the past and future statistically (as done in the present study) and later only compare the differences between the hydrological simulations for the past and the future using past and future climate simulations as input for the hydrological simulations. Figure 6 follows this direction but all other results seem to compare current and future conditions generated on the basis of different meteorological input (station observations vs RCM simulations). Using the just outlined approach the hydrological simulations would be better comparable as (hopefully) the same systematic biases are found in the simulations for the past and future. As a large number of RCM simulations are providing meteorological conditions for the past in this study, this would require defining some sort of "one hydrological reference" (e.g., the multimodel mean) the different scenario simulations can then be compared to. Apart from this main point of criticism, some further points for improvement remain, which are described in the "Specific comments" and mostly represent suggestions for clarification as well as options to make the contents of the study easier transportable to the reader.

A final issue to be mentioned is, that at some point a closer linkage of the hydrological results to the climate change signal in the applied climate scenarios for Switzerland (e.g., by showing and discussing the temperature and precipitation change) would be beneficial to interpret the presented hydrological changes. However, considering the large number of scenarios and realizations this is probably beyond the scope of the article and should hence not be an issue that needs to be addressed in the revision.

As a final recommendation, I suggest to accept the article for publication in HESS given all issues have been addressed properly (major revisions)

Reply: *Thank you for your suggestion of replacing the regimes derived from a control run (observed meteorology) by regimes derived from a reference run (simulated meteorology). We will follow your suggestion and replace the regimes derived from the control run by a multi-model mean computed from the 39 reference runs. All the figures affected will be updated accordingly.*

Specific comments (individual scientific questions/issues)

1) p.3, l.24: The authors describe existing approaches for the generation of discharge time series and then name the approach chosen in their study. Although more detailed information is provided in the "Methods section", it might be valuable for the reader to receive some brief description, in how far the applied approach differs from or matches the ones described shortly before. Maybe the authors could add some information on this here?
Reply: *We will add a brief description of the differences of the simulation approach employed here as compared to existing approaches. As opposed to classical phase randomization approaches, this approach does not rely on the empirical distribution but uses the flexible, four-parameter kappa distribution (Hosking, 1994), which allows for the generation of a wide range of realizations of high and low discharge values.*

2) p.4, Figure 1: This figure is considered very important for the reader to get an overview of the study regions. However, apart from the outlines of the study regions, the map seems rather general and would benefit from some additional detail. Maybe it would be possible to support orientation for non-EU readers e.g., by modifying Figure 1, including a larger scale overview map linked to the original map as well as by adding some more details e.g., the names of larger rivers, lakes or cities?
Reply: *We will add an inlet map of Europe, showing the location of Switzerland within Europe. Since the map is already quite busy we would prefer to restrict the labels to the ones already present.*

3) p.4, l.7: The authors include the information "second step" in brackets - this approach helps to structure the workflow and might be a good extension also for the first and following steps, which are currently referred to as part of the describing text. Having all the steps in brackets would make it easier to navigate through the workflow for the reader.
Reply: *We will add the number of the step in brackets.*

4) p.4, l.9: The discharge series used for the estimation of extreme regimes are based on hydrological simulations using observed meteorological conditions for past discharge conditions and simulated meteorological conditions (climate model output) for future discharge conditions. Given the biases in currently available climate simulations the reader here wonders if some correction has been applied before the application of the climate simulations for past and future in this study. Although this is later explained adding "bias-corrected" and "downscaled" might satisfy the readers curiosity at this early point.
Reply: *The information will be added to the text.*

5) p. 5, Figure 2: Number 1 in the workflow (comparison procedures) leads to some confusion from my point of view. I understand from the workflow description starting on p.4, l.5 that the different methods for estimating extreme flow regimes were tested, however this comparison seems to require the hydrological simulations of the left side of Figure 2 as input already, which somehow conflicts with the rank in the overall workflow.

Some further confusion in Figure 2 from my point of view arises in the context of the "Data" column. The caption says "A" introduces the simulated data used, however the two plots rather seem to show the meteorological input applied (climate simulations and observations) for the discharge simulations. On the other hand, the data flow from "A" to "C" (the "Estimation" box), as well as that from "A" to "B" requires the two data inputs from "A" to be the discharge simulations achieved on the basis of the two different meteorological data sources (climate simulations and observations). Maybe the authors could clarify these issues in an updated version of Figure 2. It thereby would be an option to use different colors for meteorological input and hydrological model results - while white and orange are used in the figure, the caption does not clarify whether the colors are used in this sense. Maybe it would also be beneficial to use different signatures/colors for the current and future estimates in box "C" and more clearly link them to the discharge simulations based on meteorological observations and climate simulations following the argumentation on p.4, l.9? Maybe also the PREVAH model, which is later mentioned to be used for the hydrological simulations, should be already integrated in the figure to complete the workflow?

Reply: *Thank you for your suggestions. Figure 2 is divided into two parts. The "Data" part and the "Estimates" part. We consider the model simulations to be part of the data generation since the main focus and innovation of our manuscript lies on the "Estimation" part. We did therefore not include the hydrological model into the figure. When we talk about data in this figure, we refer to discharge data, be it observed or simulated. This will be specified to avoid confusion.*

6) p.5, l.12: The authors describe that validation was performed for the period 1983-2005, presumably using meteorological observations as input for the PREVAH model. As climate simulations are used for the model runs for future conditions, I wonder if any validation using climate simulations for the past has been carried out beside the results shown in Figure 6 (e.g., by using hindcast simulations that reproduce weather conditions and allow a comparison of simulated and observed discharge at daily basis)?

Reply: *The hydrological model was indeed validated using meteorological observations as input for the PREVAH model (see p. 5, l:12-14). As suggested in one of the next comments, we compared the FDCs for the reference period 1981-2010 derived based on observed meteorology to FDCs derived for the same period using the simulated meteorological data generated by the 39 GCM-RCM combinations. This analysis showed that the FDCs are reproduced well in most catchments, except for the catchment Engadin. As shown in Figure 6, the model was also validated with respect to the reproduction of past low- and high-flow regimes as these were the focus of this study. We will extend Figure 6 by including the high-flow results.*

7) p.5, l.12: The authors explain that the applied hydrological model has been calibrated for the period 1993-1997. Assuming that the model was driven by meteorological observations while calibrating, the question of systematic biases due to calibration under observed meteorological conditions as well as the application of different meteorological input for the past and future arises. From my perspective both the application of different meteorological input for the past and future (meteorological stations vs climate model simulations) as well as the fact that calibration has been carried out for past observed climate conditions, whereas the simulations are presumably carried out with the same parameter set for future simulated climate conditions, need some discussion.

Reply: *It has been confirmed by Krysanova et al. (2018) who did a review study on the performance of hydrological models under climate change that a good performance of hydrological models in the historical period increases confidence in projected impacts under climate change. We found that the hydrological model well reproduces the hydrological regimes analyzed in this study in the reference period and therefore assume that it will also reliably simulate hydrological regimes under future conditions. A short discussion about this issue will be added to the manuscript.*

It might be the case that calibration compensates for some of the biases in the observed meteorological input data (e.g. an undercatch in precipitation), which are not present or at least different in the case of meteorological simulations. Particularly in snow dominated regions differences in temperature and precipitation between the two data sources can lead to different water storage in the snow pack affecting the simulated discharge conditions. A comparison of the statistical discharge characteristics achieved for the past using meteorological observations (for which the model was calibrated) to those achieved for the past using climate simulations (which were not used in the calibration) over a climatological period of time could show the differences in the discharge characteristics (e.g., using flow duration curves), which might somehow also affect the comparability of past and future discharge simulations in the present study. Maybe the authors could add some discussion on these issues to the manuscript.

Reply: *As shown in Figure 6 in the manuscript, the water balance of most catchments is well represented by using the simulated meteorological data instead of the observed ones. We followed your suggestion and did the validation also on the FDCs directly (Figure 1). The results show that the FDCs are mostly well reproduced by using the simulated meteorological data to simulate discharge. An exception is, as in Figure 6, the Engadin, where high flows are slightly overrepresented. We will discuss in the manuscript that this overestimation might be related to the univariate bias correction applied which might not perfectly reflect the interplay between temperature and precipitation and therefore the timing of snowmelt processes* (Meyer et al., 2019).

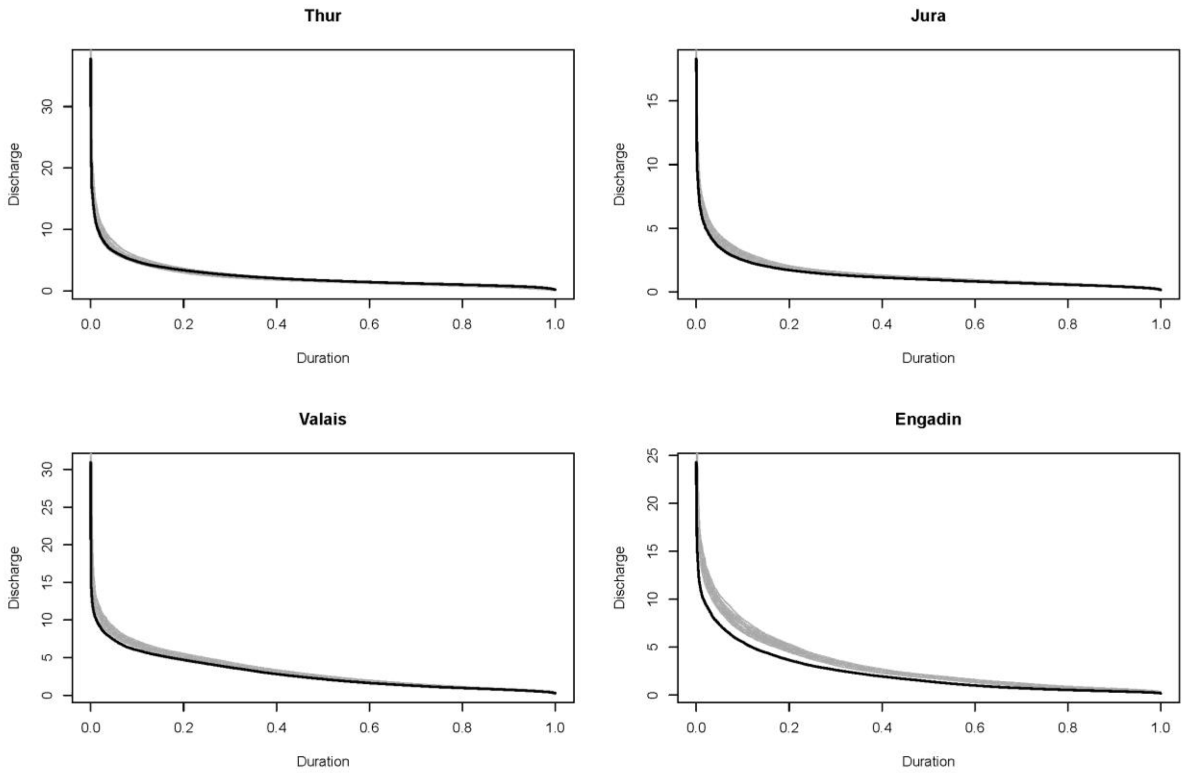

Figure 1: FDCs derived from the control run over the period 1981-2010 (black line) and the 39 reference simulations for the same period (grey lines) for the four catchments Thur, Jura, Valais, and Engadin.

8) p.6, l.2: Two glacier models are applied depending on the length of the glacier considered. It would be interesting why the extended GloGEMflow model is not applied to smaller glaciers (I understand that glacier flow is less important in this case but are there any reasons applying only the GloGEMflow model for all lengths is inadequate?) and what the differences between the results of both models would be - the latter might give an impression of the impact on the overall results induced by using the two different glacier models.
Reply: *For small glaciers a simplified glacier model is indeed used (vs. ice-dynamic model for glaciers >1 km). As shown in the original publication describing the glacier model* (Zekollari et al., 2019)*, for small glaciers the difference between the modeled glacier evolution with the simplified model and the ice-dynamic model is very small though - the difference is found to increase with increasing glacier elevation range (see Figure 12 in* Zekollari et al., 2019*).*

9) p.6, l.11: Does radiation refer to shortwave or longwave radiation or both? Please clarify.
Reply: *We will clarify that radiation refers to shortwave radiation.*

10) p.6, l.11 and l.17: Has the approach of Quantile Mapping been applied for all meteorological variables listed in l. 11? Particularly short- and longwave radiation recordings are often not available far back in the past, reducing the possibility to statistically correct the simulations for these variables. Not correcting all variables used as input for the hydrological model would explain differences between the hydrological model results achieved using meteorological observations and those based on meteorological simulations.
Reply: *The quantile mapping approach was applied for all the variables. This will be specified in the text. Neglecting the dependence between the variables leads to some deviations between the hydrological model results achieved using the meteorological observations and those using meteorological simulations.*

11) p.6., l.18: Does "model chains" here refer to "GCM-RCM combinations" - if yes, this term would be more precise and should be used alternatively. Or are "GCM-RCM combinations for different scenarios and realizations" the content behind the "model chains" - in any case, please be more precise and avoid the use of "model chains" as it can include quite a lot of pre- and postprocessing steps when used without further clarification.
Reply: *We will replace the term model chain by GCM-RCM combinations for different scenarios since a chain in our case encompasses an RCP, a GCM, and an RCM.*

12) p.6, l.19: Please rephrase to "for the locations of various meteorological stations".
Reply: *We will rephrase the sentence.*

13) p.6, l.23: Please add "for topographic corrections" or a similar completion to "was additionally used".
Reply: *We will complete the sentence as suggested.*

14) p.6, l.34: Although later specified, it is not clear whether the applied discharge series is that observed for the gauging stations or the hydrological simulations - maybe adding "(here

the simulated discharge for past and future conditions)" would make this clear as early as possible.
Reply: *We will specify this in the text.*

15) p.6, eq. 1: What does "i" stand for?
Reply: *It will be specified that i represents the imaginary unit.*

16) p.6. l.14: "Model chains" is not really precise (see comment 11), please provide additional information.
Reply: *We will replace the term model chain throughout the text and make it more specific.*

17) p. 7, l.24: Here the reader wonders how these unrealistic estimates are handled and how they affect the robustness of the study findings - would it be an option to already point out here that the univariate technique will not find further consideration in the study as later mentioned (see p. 11, l. 2)?
Reply: *This information will be added to the text at this early point of the manuscript.*

18) p. 7, l.3: Does this mean the typical flow regime assumed is allowed to be different for past and future conditions depending on the simulated discharge time series - this is an important point and needs to be included.
Reply: *This point will be included.*

19) p. 8, l. 6: Is the number necessarily as high as 1500 years to apply this approach or is it 1500 years because the authors carried out simulations for 1500 years (see p.7, l. 11)? Maybe adding "here" before 1500 would avoid any speculation.
Reply: *The approach is very flexible as to how many simulations are performed. We here run the simulation procedure for 1500 years, which will be pointed out in the text.*

20) p.8, l. 13: The authors describe that a "control regime" was generated on the basis of discharge simulations achieved using meteorological observations as input for the hydrological model and that a number of "reference regimes" have been derived from the hydrological simulations based on the different GCM-RCM combinations, resulting in a range of current regime estimates. This is not fully in line with previous statements that describe that the discharge simulations representing past discharge conditions are based on meteorological observations (see p.4, l. 9) so it would be beneficial to modify the statements at p.4, l. 9 accordingly.
Reply: *We will extend the statement by saying that we also simulated discharge for current conditions using meteorological input from a set of climate models.*

21) p.8, Figure 3: Please add to the caption that this figure is just a schematic illustration of the comparison approach to make clear the underlying data is not generated in this study and should not be interpreted.
Reply: *We will add to the caption that the figure is just an illustration.*

22) Figure 4 & 5: For me it is not quite clear whether the extreme regime estimates for this control setting are derived based on simulated discharge (see p.8, l.13) or on the discharge observations also illustrated in Figure 4 and 5. However this makes a difference, as using discharge observations for the extreme regime estimation would represent a "perfect setup" to only quantify the uncertainties in the estimation approaches, while using discharge simulations based on meteorological observations (previously defined as "control conditions") would rather illustrate the uncertainty in the whole control setting (which according to the described study design includes the uncertainties from the hydrological modelling using meteorological observations as input) as well as the uncertainties from the individual regime estimation method. Ideally, the extreme regime estimation methods would also be tested using the reference simulations (achieved using climate model data for the past as input for the hydrological model) to show the uncertainty additionally induced by applying climate simulations as hydrological model input (as done for low-flow conditions in Figure 6 using only the FDC method).
Reply: *Figures 4 and 5 are based on discharge derived from observed meteorological data in order to assess the usefulness of the different estimation approaches. This will be specified in the Figure and its caption. The choice of the extreme-regime estimation method was taken based on this comparison. The suitable methods (FDC and stochastic simulation) were afterwards applied to the reference simulations (hydrological model driven with simulated meteorological data). The uncertainty coming from this is shown in Figure 6. We will extend the Figure by the high-flow conditions to provide some information on the method's performances for high-flow conditions.*

23) p. 11, Figure 6: This figure is nice as it for the first and only time compares the results achieved for meteorological observations (control simulations) and the climate simulations - is there any reason this is only done for low-flow conditions? There also is some room for improvement with respect to the graphical realization: The different signatures show the "control simulations" (observed meteorological input for the hydrological model) and "reference simulations" (data from different GCM-RCM combinations as input for the hydrological model). The description "Control simulation" and "Climate simulations" is a little confusing, as for the control simulations the word simulations represents hydrological simulations, while in the case of the climate simulations the word simulations refers to the meteorological input applied. I therefore suggest denoting the signatures "control simulations" and "reference simulations" with a detailed description (as already provided) in the figure caption. Moreover, I would suggest to choose a dashed-type of signature for the upper and lower borders of the reference simulations (climate simulations) similar to the future range in Figure 7) to make them distinguishable from the control simulations.
Reply: *We will complement the figure with the high-flow regime estimates. The Figure legend will be adjusted as suggested by the reviewer. The borders of the polygons will be converted to dashed lines.*

24) p. 11, l.5: I would in general avoid the word "model chain" and replace it with something more precise (e.g. GCM-RCM combination). "Model chain" is a rather wide term that can

include a hydrological model, the extreme regime estimation or the downscaling and bias-correction procedure (see also comment 11).
Reply: *As mentioned previously, we will replace the term model chain by a more specific term throughout the document.*

25) p. 11, l. 8: Better replace "observed data" with "meteorological observations" to be more precise. I would also replace "means" with "suggests" as it is rather a hint in that direction and not a fact (also the line is in the spread range, the individual reference simulations can be quite far from the line).
Reply: *We will replace the words as suggested by the reviewer.*

26) p. 11, l. 9: The authors explain the overestimation in summer with an overestimation in RCM-simulated precipitation. This is plausible, but shouldn't the precipitation statistics in the RCM data match that of the observations after application of the quantile mapping approach? Maybe the authors could deepen the discussion on this issue in the updated version of the manuscript.
Reply: *The overall precipitation should indeed match the observations. However, precipitation and temperature have been bias corrected in a univariate manner.* Meyer et al. (2019) *recently showed that bivariate (temperature and precipitation) bias correction might be preferable in mountainous catchments where the interplay between temperature and precipitation has a significant impact on snow accumulation and therefore on the seasonality of discharge. This discussion point will be added to the manuscript.*

27) p. 12, Figure 7: I assume that the current regime is based on the hydrological simulations achieved using meteorological observations as input for the hydrological model (as described earlier in the text, see p.4, l.9). I see a certain weakness of this study in directly comparing the hydrological simulations achieved with meteorological observations (current) and climate simulations (future). While the applied bias correction fits the statistics of the climate simulations for the past to those of the observations (but also only in case of those meteorological variables that are bias corrected, see comment 10), the data sets still are not identical for the past and can be expected to induce different hydrological reactions, particularly when it comes to extreme events. I think the study would have benefitted from using a control regime that is also based on climate simulations, in this case for the past (e.g., multi-model mean of all hydrological model results achieved using different GCM-RCM combinations). Otherwise the systematic differences in the driving data make the results hard to compare. This at least needs to be discussed in the updated manuscript.
Reply: *Thank you for this suggestion. We agree that a comparison of the future regime estimates with an estimate derived using the control run is not ideal. We will follow your suggestion and use a multi-model mean derived from the 39 GCM-RCM combinations instead as a reference for the current climate. This multi-model mean will be used in the updated figures and to compute the differences between future and current regime estimates.*

28) p.12, Figure 7: The solid (current) and dashed lines (surrounding the shaded areas) in the case of the "mean conditions" are black in the legend but seem to be grey in the charts - this should be corrected. The caption indicates that the "normal" regimes are provided as a reference in grey, but are these the same as the “mean conditions”, if yes please try to avoid using different words for the same content to make the figure easier to understand. The legend should in this case also be modified to link a grey solid line to the current state and grey dashed lines to the boundaries surrounding the grey areas as described in the caption. Finally, the areas where orange and light-blue areas overlap are extremely hard to distinguish from light-grey areas making the plots extremely hard to read. Please try to update the plots to make them easier to read - maybe there is simply too much information in them? Separating mean and extreme conditions would already make a big difference. Moreover, rather or additionally discussing the multi-model mean instead of the spread in future discharge achieved from all GCMRCM simulations could reduce the displayed information for the sake of readability and interpretability.
Reply: *The legend will be corrected. We will use the term mean instead of normal consistently throughout the manuscript. The readability of the figure will be improved by removing redundant axis labels to save space and by replacing the thick dashed lines around the polygons by thin dashed lines. We would prefer to not separate normal from extreme conditions since the regimes derived for mean conditions serve as a reference.*

29) p.12, l.3: I would suggest to rephrase "These chains" to "Here, the different realizations".
Reply: *The sentence will be rephrased.*

30) p.12, l.3: There seems to be also a distinct reduction in mean and extreme conditions for FDC in both rain dominated catchments in spring. Apart from RCP4.5 in the Jura catchment this reduction is clearly evident in both rain dominated catchments and all scenarios. Maybe this could also be an issue of discussion.
Reply: *This observation will be added to the text.*

31) p.12, l.8: Replace "differences in" with "differences between".
Reply: *The replacement will be conducted.*

32) p.12, l. 9: Why has RCP4.5 been excluded from Figure 8. If it is because you expect the changes to be in between those resulting from applying RCP2.6 and RCP8.5 please add a short sentence including this information to the text.
Reply: *We did not include the results for RCP 4.5 to increase the readability of the plot. The results indeed lie in between those of RCP 2.6 and RCP 8.5. A short sentence providing this information will be added to the text.*

33) p.13, l. 1: Do you mean "different" instead of "distinct" - "different" would be better from my point of view as you explain how the changes differ in the following lines.
Reply: *Distinct will be replaced by different.*

34) p.14, Figure 8: "Mean" is used in the plot and "normal" is used as a describing term in the caption - I would suggest using one of the terms as the counterpart to "extreme consistently in the figures and throughout the manuscript. Please rephrase "the second three rows" to "the last two rows" in the caption as there are only five rows in total.
Reply: *Mean will be used instead of normal consistently throughout the document. The sentence will be rephrased.*

35) p.14, l.2: I would suggest to include a brief description of the main findings from Figure 9 before moving on to Figure 10.
Reply: *A short sentence will be added the make the transition between Figures 9 and 10 more fluent. The detailed results are discussed after having displayed Figure 10 since it is hard to determine actual changes from Figure 9.*

36) p.14, l.3: Are the changes really "independent of the estimation technique used (FDC/stochastic)" or just in a similar order of magnitude or comparable?
Reply: *We agree that similar is more appropriate and will change the wording.*

37) p.15, Figure 9: As Figure 7 and Figure 9 share many characteristics, the options for improvement described for Figure 7 (comment 28) mostly also apply to Figure 9. Moreover, the y-axes in the case of the Thur catchment need adjustment towards higher maximum y-values as the maxima are cut out in the case of all scenarios.
Reply: *We will adjust the figure according to the changes also made for Figure 7 (see response above). The y-axes of the Thur catchment will be adjusted.*

38) p.16, Figure 10: Please rephrase "the second three rows" with "the last two rows" in the caption as there are only five rows in total.
Reply: *The caption will be rephrased.*

39) p.17, l.17: Better change "include future climate scenarios" to "include the assumptions underlying the applied future global climate scenarios". Maybe also change "the choice of a hydrological model" to "the uncertainties inherent in the hydrological model results".
Reply: *The sentences will be rephrased.*

40) p.17, l.33: Please replace "regions but the low-flow" with "regions but with the low-flow".
Reply: *The passage was rephrased.*

Technical corrections (typing errors, etc.)

1) p.2, l.6: Better rephrase to "... in the future ...".
Reply: *This will be rephrased.*

2) p.5, l.9: I think "glacier melt" in two separate words is the most commonly used form.
Reply: *The word will be changed.*

3) p.18, l.23: Please replace "which coincides" with "which coincide" and "high flow" with "high-flow".
Reply: *The phrasing will be changed.*

References used in the answers to the reviewers

Hosking, J.R.M., 1994. The four-parameter kappa distribution. IBM J. Res. Dev. 38, 251–258.

Krysanova, V., Donnelly, C., Gelfan, A., Gerten, D., Arheimer, B., Hattermann, F., Kundzewicz, Z.W., 2018. How the performance of hydrological models relates to credibility of projections under climate change. Hydrol. Sci. J. 63, 696–720. https://doi.org/10.1080/02626667.2018.1446214

Meyer, J., Kohn, I., Stahl, K., Hakala, K., Seibert, J., Cannon, A.J., 2019. Effects of univariate and multivariate bias correction on hydrological impact projections in alpine catchments. Hydrol. Earth Syst. Sci. 23, 1339–1354. https://doi.org/10.5194/hess-2018-317

Zekollari, H., Huss, M., Farinotti, D., 2019. Modelling the future evolution of glaciers in the European Alps under the EURO-CORDEX RCM ensemble. Cryosph. 13, 1125–1146. https://doi.org/10.5194/tc-13-1125-2019